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ABSTRACT

This paper is based on the work of Thomas Kuhn, a physicist turned philosopher and historian of science. In his book, "The Structure of Scientific Revolution," he posited a new concept of the nature and history of science and strongly criticized the current textbook tradition of science education. This study analyzed Kuhn's concept of science, examined his criticism of science teaching, and synthesized the responses of some of his critics for speculative insight into related issues of curriculum development, student attitude toward science, and teacher training. The analysis revealed a perceived incompatibility of many facets of the current textbook tradition with a historical approach to science teaching. (TW)

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ANALYSIS OF THE EDUCATIONAL IMPLICATIONS OF THE CONCEPT OF SCIENTIFIC REVOLUTIONS

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(Abstract) (Abstract)

ANALYSIS OF THE EDUCATIONAL IMPLICATIONS OF THE CONCEPT OF SCIENTIFIC REVOLUTIONS

The work of Thomas Kuhn, a physicist turned philosopher and historian of science, has been a pivotal factor in the growing concern that the picture of science presented in the typical school or college classroom may be incompatible with either the nature of science or society's educational needs. In The Structure of Scientific Revolutions, he posited a revolutionary new concept of the nature and history of science and strongly criticized the current textbook tradition of science education. From the history of science, Kuhn distinguished a genetic dichotomy in scientific development which he described as "normal" scientific activity punctuated by relativly brief episodes of "revolutionary," or abnormal, scientific activity. Normal scientific development generally prevails, he concluded; but revolutionary episodes of noncumulative developmental activity, during which the normal research tradition may be displaced by an incompatible new one, also characterize the history of science. However, he charged, science textbooks typically depict these revolutionary developments as cumulative additions to scientific knowledge.

Most science students therefore receive a tradition-bound, positivistic, perspective of science as a routine, accretionary enterprise, Kuhn declared; but, he contended, this sort of science never existed. Because science textbooks are perceived

as authoritative sources of scientific information by scientists and laymen alike, he conjectured, the cumulative effect of the ahistorical textbook tradition is compounded among succeeding generations of science students. On that basis, he decried the dominance of the textbook in science teaching.

Structure has drastically altered the way the nature and development of science is perceived by the scientific community, and it clearly challenged a fundamental dimension of the science curriculum. This study analyzed Kuhn's concept of science, examined his criticism of science teaching, and synthesized the responses of some of his critics for speculative insight into related issues of curruculum development, student attitudes toward science, and teacher training.

ANALYSIS OF THE EDUCATIONAL IMPLICATIONS OF THE CONCEPT OF SCIENTIFIC REVOLUTIONS

Introduction

For the past three decades, science education has been expected to produce both the scientists and the scientifically literate society required to maintain a science-oriented civilization. Many scientists were produced during that time, but several national surveys during the 1970s indicated that "the level of scientific literacy in the United States is deplorably low" (Miller, 1983, p.29). Reflecting on the importance of a public understanding of science in a technological society, Brovey (1980) expressed a common concern among scientists and educators: "We cannot survive as a democracy, as a civilization, without citizens who are scientifically literate" (p. 91). From a review of studies on the current problems in science education, Gardner and Yager (1983) concluded that "there is a crisis in science education! Almost everyone is willing to concede that such a condition exists and that something must be done" (p. 587).

Speculation has abounded regarding ways that science education might be reformed. Most of the discussion focused on curricular or instructional issues related to how science is taught, but some writers implied that the problem may be more closely related to what is taught as "science". Evans (1970), for example, noted that the science courses taken by teachers "were generally designed for neophyte scientists" (p. 82) and may not present an

adequate picture of the scientific enterprise. Rigden (1983) also indicated that a general public misperception of the nature of science and the work of scientists "stems from misconceptions that originate in the classrooms of schools and colleges" (p. 613). Furthermore, he contended, most science teaching is based on textbooks that present "a distorted picture of science" (p.615).

The research of Thomas Kuhn, a physicist turned philosopher and historian of science, is especially relevant to the growing concern that the picture of science presented in the typical classroom may be incompatible with the nature and history of science and therefore inadequate for society's educational needs. In The Structure of Scientific Revolutions, originally published in 1962 and revised in 1970, he posited a revolutionary new concept of the nature and history of science and strongly criticized the current textbook tradition of science education as inefficacious.

Purpose of the Study

The purpose of this study was to analyze Kuhn's concept of the nature and history of science, examine his perception of science teaching, and synthesize the responses of some of his major critics for insight into the educational implications of his ideas.

The Procedure

Philosophical research methods are uniquely appropriate for initial enquiry into broad problem areas to synthesize points of view and illuminate additional options or generate new ideas that may be compatible with the basic purposes and ideals of an institution, agency, or group. Speculative philosophical enquiry was therefore employed to elucidate some of the implications of Kuhn's historiography of science for science education.

Observations from the Study

The traditional positivistic philosophy of science required neutral observational data for the adjudication of rival research traditions. The main theme of The Structure Of Scientific Revolutions (hereinafter referred to as "Structure") is that scientific positivism should be replaced by an epistemological system wherein the ultimate choice between rival research traditions, or "paradigms," is attributable to "no standard higher than the assent of the relevant community" (Kuhn, 1970, p. 94). Over the past quarter-century, this controversial concept has drastically altered the scientific community's perception of the nature and history of its enterprise and has challenged the traditional approach to science education.

The Kuhnian Controversy

Like other pivotal works, Structure met with enthusiastic acclaim in some camps and scathing denunciation in others. It has become "nothing less than a bible for countless scientists, historians, sociologists, and psychologists, and has been treated as nothing less than heresy by most philosophers of science" (Cedarbaum, 1983, p. 174). Within a decade of its publication, one of Kuhn's major critics conceded that Structure had become "one of the most popular attempts of all time to interpret the nature of science" (Shapere, 1971, p. 706). Reflecting on the controversy surrounding his book, however, Kuhn declared that:

> I have sometimes found it hard to believe that all parties to the discussion had been engaged with the same volume. Part of the reason for its success is, I regretfully conclude, that it can be too nearly all things to all people. (Kuhn, 1977, p. 293).

Scheffler (1972) and Ruse (1970), for example, interpreted Kuhn as an out-and-out opponent of objectivity in the scientific enterprise, whereas Mieland (1974) perceived him as "not an opponent but a proponent" (p. 179). Others saw Structure challenging the traditional concepts of the history and nature of science itself. Gillispie (1962), for example, perceived it as a Darwinian historiography of science. Many differences of opinion, Mieland surmised, may be simply a question of the way the reader approaches an issue. For example, if one examines the principles of paradigm change from the context of anomaly, Kuhn's

ideas are consistent; but if approached from the perspective of the charge of irrationality, "one sees Kuhn's book as .nconsistent" (Mieland, 1974, p. 187).

Kuhn attributed much of the problem to his use of the term "paradigm" to illuminate the role of tradition in the scientific community. "Paradigm" as he used it, he explained in a postscript to the second edition of Structure, "is what the members of a scientific community share, and, conversely, a scientific community consists of men who share a paradigm" (Kuhn, 1970, p. 176). This problem might have been reduced, he implied, if the book had opened with a sociological explication of the structure of scientific communities. Because of the unclear use of the term, Cedarbaum (1983) concurred, "the book which Kuhn believed he had written is virtually unknown" (p. 174). "Few books have been as widely read and as widely misunderstood as Structure" (p. 174), he concluded; and because it is far more complex than its aphilosophical facade projects, "no adequate appraisal of the book has been written" (p. 175).

Much of the criticism directed at Structure, is probably a product of the brevity of the publication. By covering the history of science and restructuring the whole fabric of scientific epistemology in 200 or so pages, Kuhn admittedly painted his concepts with broad strokes and left much open for further explanation. Nevertheless, even Kuhn's staunchest critics generally concur with Shapere (1971) that "Kuhn's book has had an undeniably healthy influence on discussions of the nature of

science" (p. 706). Structure has compelled many scientific thinkers "to face for the first time the full profundity of the conceptual transformations which have, at times, marked the historical development of ideas" (Toulmin, 1970, p. 39).

Kuhn's Concept of the Nature of Science

From an extensive examination of the history of science, Kuhn distinguished a genetic dichotomy in its nature which he described as "normal" and "revolutionary" episodes of scientific development. "Normal science" consists of stable, productive periods of cumulative scientific enquiry conducted within an established research tradition, or paradigm. Normal scientific development, "the activity in which most scientists inevitably spend almost all their time" (Kuhn, 1970, p. 5), is occasionally disrupted by periods of "revolutionary science", or episodes of noncumulative developmental activity in which an unsatisfactory paradigm is replaced, often with chaotic ramifications for the scientific community, "by an incompatible new one" (p. 92).

A scientific paradigm, Kuhn explained, is a research tradition consisting of "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" (Kuhn, 1970, p. viii). It functions to: (1) define the field of study; (2) focus the investigation on relevant facts; (3) define the experiments needed to articulate the relevant theories; (4) provide the rules for research; and (5) help assure success in the enterprise. Ideally, paradigms

set the scientific problems to be researched, prescribe the puzzles that must be solved to give coherence to the conduct of science, and are "prerequisite to the discovery of laws" (p. 28).

Paradigms are not necessarily discipline-specific, nor is any single paradigm critical to the entire scientific enterprise. If the languages of the supporting scientific communities are not too disjointed, Kuhn (1970) noted, a scientist may be committed to several paradigms. Also, a paradigm may be shared by several scientific communities, as in Newtonian dynamics, but "it is not the same paradigm for them all" (p. 50). Because of the nature of the language each community requires for conceptualization of its ideas, proponents of competing paradigms generally "practice their trades in different worlds" (p. 150). Each sees the same things so differently until their undergirding theoretical systems and principles are incompatible.

"New paradigms are born from old ones," Kuhn (1970) declared: and they often incorporate many of the concepts and methodologies of the old paradigm, "but they seldom employ these borrowed elements in quite the traditional way" (p. 149). For example, after Copernicus, the terms "fixed position", "orbit", "planet", and "satellite" remained in the vocabulary of astronomy, but they were applied quite differently with regard to the Sun, Moon and Earth. Likewise, the term "force", employed in both Aristotle's and Newton's physics, underwent a drastic definitional change from one paradigm to the other. But the old paradigms as such were not changed, Kuhn contended, because the theoretical

incommensurability between the old and new ideas was too great. Rather, the old paradigms were destroyed and their useful parts salvaged for use in new models, but not in the cumulative, positivistic sense in which scientific progress has traditionally been been perceived.

> The whole process, Kuhn finally concludes, is not cumulative in the sense of progress towards truth about nature, but is rather progressive in the evolutionary sense of natural selection out of competing situations.
>
> (Hesse, 1963, p. 286)

Scientific progress should not be viewed as a tradition driven process toward what may be known, Kuhn concluded from his study of the history of science, but rather as a goal-driven process toward what the community of scientific thinkers wants to know. A paradigm may be considered superior to its competitors on the basis that it is a better problem solver, but this does not mean that it necessarily reflects more clearly what nature is really like--only that it more effectively helps its adherents learn what they want to know about nature from the perspective of that research tradition. Consequently, he argued, there can be no guarantee that scientists today know more about nature than their predecessors, only that they know more about what they want to know about nature within the epistemological constraints of the currently accepted world-view. "We may, to be more precise, have to relinquish the notion, explicit or implicit, that changes of paradigms carry scientists and those who learn from them closer and closer to the truth" (Kuhn, 1970, p. 170).

Kuhn conceived scientific development as "a process of evolution from primitive beginnings--a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature. But nothing that has been or will be said makes it a process of evolution toward anything" (Kuhn, 1970, p. 170-171). Rather, scientific development may occur through either of two distinct processes: normal scientific activity within a paradigm, which is similar to the traditional perception of cumulative scientific progress; or through abnormal, or revolutionary, scientific activity in the absence of a satisfactory research tradition.

Revolutionary science activity may ensue when some members of a scientific community find themselves--as did Darwin and his peers--faced with novel data that is incompatible with the normal research tradition but which they cannot justifiably ignore or dismiss as extraneous. In most cases, the concerned scientists will explore alternative approaches to such problems within the context of relevant theory, Kuhn explained; but they are seeking to perfect the theory--they are "not testing the paradigm" (p. 144). If the alternative approaches (Kuhn calls them ad hoc modifications or epistemological counterinstances) cannot readily handle the problems, however, an open-minded observer may suddenly propose a radically new, speculative approach to the question--as Copernicus did while trying to refine Ptolemy's explanation of planetary motion. If the new research strategy appears plausible to a sufficient number of dissatisfied scientists, they may form a core community for testing it.

If the new approach satisfactorily handles the novelty and also provides plausible answers for some of the more important prior problems that gave the old paradigm its credence, the new scheme may be perceived as the basis for a better paradigm. The scientific community will then find itself in a crisis, divided over competing paradigms. It must either keep the old paradigm and reject the new upstart, or adopt the new approach and discard the old one as no longer productive. Otherwise, it will find its scientific tradition susceptible to the challenge of incoherence and indeterminism. The methodological desiderata of determinism and coherence in science renders the latter option unacceptable, however, and the stage is set for a possible scientific revolution as members of the community align themselves with one or the other of the competing paradigms. During crises, Kuhn (1970) noted, scientists commonly turn to philosophical analysis--which "normal science usually holds . . . at arm's length, and probably for good reasons" (p. 88)--in order to explore as many alternatives as possible. Consequently, a "crisis often proliferates new discoveries" (p. 88) by providing an opportunity for uncharacteristic freedom of scientific inquiry.

Although paradigms are not tested in the process of normal science, Kuhn concluded, they are tested severely during episodes of revolutionary science. If the "old school" of scientific thought prevails the old paradigm will survive, probably somewhat enriched if it assimilated some of the more compatible ideas of the now disgraced upstarts. If a "new school" of thought gains enough support, however, the old paradigm may be castigated and

finally discarded as a failure. Therefore, he contended, selection of research traditions is much more than a simple matter of the rational choice of the superior alternative on the objective basis of its logical consistency and empirical problem-solving power. A third factor, specifically the consensus of the relevant scientific community, is a critical factor in the direction of a field of science.

Kuhn's contention that science is not purely methodological, but is also susceptible to external social factors, led some of his critics, including Shapere, to decry his system as epistemologically relativistic and incapable of providing a rational basis for scientific progress (i.e.: for determining whether the paradigm that finally prevails is the "right" alternative). Again Kuhn turned to the history of science to support his position. The presupposition that science has drawn constantly nearer to ultimately discernable truths--the "right" facts, natural or otherwise--about nature appears to be an illusion based on a fundamental misinterpretation of the function of the scientific enterprise, he countered; in which case the charge of relativism would be similarly unfounded.

> What must nature, including man, be like in order that science be possible at all? Why should scientific communities be able to reach a firm consensus unattainable in other fields? Why should consensus endure across one paradigm change after another? And why should paradigm change invariably provide an instrument more perfect in any sense than those known

> before? . . . That problem--What must the world be like in order that man may know it?--is as old as science itself, and it remains unanswered.
>
> (Kuhn, 1970, p. 173)

Scientific revolutions are imaginative and creative episodes of such impact that "afterwards science transpires in a new world" (Gillispie, 1962, p. 1252). They represent "a reconstruction of the field from new fundamentals" (Kuhn, 1970, p. 85). In a normal (one-paradigm) science situation, practitioners have an established scientific tradition upon which to base their enquiry and justify their conclusions. In a revolutionary situation, however, the security of an incontestably authoritative basis for research disappears. In an effort to re-establish a semblance of normalcy, a desperate search for a credible alliance may ensue as competing paradigms are considered. Individual commitments to one or the other alternatives generally become quickly and deeply entrenched, perhaps emotionally so. Some scientists may choose to reject a new world outlook completely, as many did when confronted with Darwin's theory of evolution. Others who find portions of an old paradigm unsatisfactory may choose to translate a competing paradigm into their own language, Kuhn noted, as did those peers of Copernicus who chose to employ his planetary concepts whenever they produced better results. Still others may subscribe totally to a new world-view long before it is established, as Galileo did when he endorsed the heliostatic concept.

Given the absence of a positivistic basis for a systematic selection of alternatives, however, the decision to switch paradigms--to internalize a new research tradition--is not a simple matter of rational choice. For an individual, it may rather resemble a "conversion experience . . . likened to a gestalt switch" (p. 204).

> The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith.
>
> (Kuhn, 1970, p. 158)

The Normal Basis of Scientific Revolutions

Reviewing the history of several major developments in science, Kuhn concluded that once an episode of revolutionary scientific activity culminated in a new research tradition, the supporting scientific community regained its normal confidence and set about tackling new problems and reiterating the solutions to old problems in the language and framework of the new research model. With time, the revolutionary paradigm became the basis of a new era of normal scientific enquiry as its adherents accomplished the puzzle-solving and mop-up activity necessary to establish it. Stability prevailed again, based on the presupposition that, by

virtue of its current paradigm, "the scientific community knows what the world is like" (Kuhn, 1970, p. 5).

Once again there was an accepted standard for determining those questions that constituted an appropriate basis for scientific enquiry and for dismissing any that were "unscientific." Once again, research focused on the normal science activity of authenticating the match of observed fact and the predictions of the established paradigm.

> Normal science does not seek novelties of fact or theory and, when successful, finds none. New and unsuspected phenomena are, however, repeatedly uncovered by scientific research, and radical new theories have again and again been invented by scientists. History even suggests that the scientific enterprise has developed a uniquely powerful technique for producing surprises of this sort.
>
> (Kuhn, 1970, p. 52)

That powerful technique for producing radical scientific change is normal science itself. If nature cannot be fit completely into the new paradigmatic framework, and inevitably there will be misfits in any save a universally perfect paradigm, then anomalous observations or surprises may accrue and yet another episode of revolutionary science may follow. Every datum that cannot be readily accommodated therefore constitutes a challenge to the prevailing research tradition. Thus, for Kuhn, nature itself is the ultimate check of the efficacy of a paradigm; and

revolutionary scientific activity is the means through which science progresses from inadequate research traditions to ones more compatible with nature as it is perceived within the prevailing world-view. As Tolstoy (1981) noted, a normal science tradition is the source of its own undoing as it inevitably raises awkward questions that it cannot handle.

Textbook Distortion of the History of Science

Revolutionary episodes of development have been frequent in the history of science, Kuhn concluded, and he convincingly supported his position through a concise historical examination of several examples such as the development of oxygen theory. However, he lamented, in the textbooks that typify science education, these episodes "have customarily been viewed not as revolutions but as additions to scientific knowledge" (Kuhn, 1970, p. 136). Therefore, the existence and significance of many of these episodes have been systematically obscured in most school and college classrooms.

Because the immediate productivity of the scientific enterprise is dependent upon the inculcation of the currently accepted research tradition or paradigm, Kuhn (1970) explained, scientific training has almost exclusively focused on the teaching of the current problems and theories of science, with only cursory mention of a few historical generalities that may lend credence to the current paradigm. Consequently, "scientific education makes use of no equivalent for the art museum or library of

classics, and the result is a sometimes drastic distortion in the scientist's perception of his discipline's past" (p. 167).

> For reasons that are both obvious and highly functional, science textbooks (and too many of the older histories of science) refer only to that part of the work of past scientists that can easily be viewed as contributions to the statement and solution of the text's paradigm problems. Partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific.
>
> (Kuhn, 1970, p. 138).

That textbook derived image of science, he argued, "is one that, in fact, never existed" (Kuhn, 1970, p. 138). But unfortunately, he elaborated, science textbooks are perceived as authoritative sources of scientific information by scientists and laymen alike. "More than any other single aspect of science, that pedagogic form has determined our image of science and of the role of discovery and invention in its advance" (p. 143). And it has cumulatively perpetuated that misperception of the nature of science, he lamented, to the point that it is common throughout society, including the scientific community.

The distorted image of science decried in Structure "has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in . . . the science textbooks from which each new scientific generation learns to practice its trade (Kuhn, 1970, p. 1). Because of the scope and complexity of the scientific enterprise, textbooks can present at best only a very limited perspective of science. Like tourist brochures which must be rewritten as cultural conditions change, Kuhn explained, science texts must be rewritten whenever there is a significant change in scientific traditions. That repeated editing produces a limited, factual perspective of science that is largely devoid of the challenging events that had established its credibility within the scientific community. Therefore, "most science instruction ignores the thrill of the quest for knowledge" (Rigden, 1983, p. 613). Consequently, Kuhn concluded, scientists and the work they do are too commonly perceived as routine, mundane, unimaginative, and irrelevant.

Some Implications for Science Education

Educators and scientists have written extensively about the problem of scientific illiteracy, and the condition has been ascribed to a failure of science education to provide the instruction students need to understand and appreciate science. There has been increasing concern that the discipline-centered, theory-specific emphasis that has prevailed in science teaching since the mid-1950s may be a major contributing factor to this

problem. The possibility that the history of science may be a key to improved science instruction has been widely discussed by writers such as Brush (1974), Kauffman (1980), Klopfer (1969), Patterson (1980), Russell (1981), and Siegel (1979). Frequent reference to Structure in these and similar publications indicated that Kuhn's ideas on this issue should be carefully examined by advocates of reform in science teaching.

Inclusion of the history of science in the curriculum would do much to produce a better understanding of science, Kuhn declared, but educators attempting to do so must deal with several serious problems. For example, it could conceivably take as long to teach the principles of an outdated and normally useless paradigm as it would its modern, scientifically productive counterpart, thereby possibly doubling the required time for instruction. Given the limited time available for science instruction in the typical curriculum, he wondered, how may the historical detail needed for a better general understanding of the nature of science be balanced with the technical, theoretical detail necessary for would-be scientists to function effectively in the current normal tradition? Would teachers be able to do justice to both the timeless questions of the nature and history of science on the one hand, and the timely problems of the current research paradigm on the other?

What should be taught from the history of science? Should astrology and mythology, for instance, be covered in an astronomy course? Should the concept of static creation be taught prior to

introducing the concept of evolution in a biology course, or catastrophism before uniformitarianism in a geology course? How much detail should be devoted to outcast paradigms? Should they be taught objectively or as viewed by the current scientific community? Should the emphasis be limited to illuminating the differences between paradigms, or should it also focus on the political and social implications of respective scientific points of view? Also, what sort of science history should be employed to train science majors; and how should it differ, if at all, from that for non-majors? Review of these and related issues posed by Structure may provide additional insight into the comparison of general science programs and two-track science curricula, as discussed by Gardner and Yager (1983).

Kuhn conceded that aspiring scientists may be more efficiently taught with the traditional textbook approach, since they could more quickly master the currently acceptable paradigm and become productive normal scientists. Siegel (1979) strongly criticized Kuhn's concession and cited evidence that courses presenting the history of science in an "unKuhnian, i.e., undistorted, way" (p. 111) "can be and are successful in producing competent scientists" (p. 117). Furthermore, he declared, there is convincing evidence that inclusion of the history of science in physics courses produces a higher degree of student satisfaction. Like some of the other criticisms of Kuhn's work, Siegle's concern may have been precipitated by a too brief treatment of a complex problem in Structure. Whereas Kuhn's acknowledgement of possible curricular benefits of distorting the history of science for some

training purposes was perceived as support of that position by Siegel, Structure's lamentations of the overall deleterious effects of that practice appeared to provide equally convincing evidence for the opposite conclusion. In any case, Kuhn clearly advocated that science students, aspiring scientists and non-majors alike, who were taught from the historical perspective would more fully understand the nature and meaning of the science they studied.

Kauffman (1980) presented evidence that, with the necessary care, science history can be effectively integrated into chemistry courses. Rather than simply serving as technical reporters of modern science, he concluded, history-oriented teachers may interpret science to students and provide sufficient supporting historical information for them to value science for themselves. Quoting a colleague, he concurred that "while it is possible to train a chemical technologist without giving him a knowledge of history of chemistry, it is difficult to educate a creative chemist without such knowledge" (p. 26).

Given the current concern that science teaching be made commensurate with the needs of all students, Kauffman's and Siegel's reports present a case for further study of the incorporation of the history of science into all facets of science education. Patterson (1980) warned, however, that overzealous treatment of names and dates should be avoided in the teaching of science history. That would not only alienate many students, she concluded, it would also do violence to both

history and science as disciplines. Rather, the history of science should be correlated with the traditional normal science content in such a way as to help students observe how scientific ideas and methods change over time "and observe concurrent changes in man's society, his material environment, and his intellectual outlook" (p. 17).

Brush (1974) presented a case for the historical approach with three observations: (1) it will give teachers a chance to discuss conceptual problems that conventional science teaching overlooks, or deliberately by-passes; (2) it might help to recruit for science careers some potential Darwins who would not perceive the relevance of science through the orthodox science training process; and (3) the study of science history may "give some hints on how to survive the recent public hostility to science" (p. 1171). Quoting the science historian, D.S.L. Cardwell, Brush concurred that:

> the history of science could aid the teaching of science by showing that "such puzzling concepts as force, energy, etc., are man-made and were evolved in an understandable sequence in response to acutely felt and very real problems. They were not handed down by some celestial textbook writer to whom they were immediately self-evident."
>
> (Brush, 1974, p. 1171)

Other possible affective benefits from the history of science were revealed in the literature. Etzioni and Nunn (1974)

reported a decline of public confidence and appreciation of science amidst growing public concern that the modern scientific community is uncaring, insensitive, and antagonistic toward human values and sociocultural conditions. According to Brush, this is remarkably similar to the image of scientists as highly objective, socially aloof robots lacking either human values or emotions that is presented in the typical science textbook. The possibility that the inclusion of history of science in the curriculum might help students to perceive scientists in a more realistic and favorable light, as Kuhn indicated it would, certainly deserves careful examination. If so, it would do much to reduce the possibility of a disillusioned modern society rejecting its scientists "as their ancient forebears did their unsuccessful witch doctors" (Kauffman, 1980, p. 27).

Brush predicted, however, that the ahistorical textbook tradition in science education will not be easily overcome. The typical text presents a streamlined picture of science stripped of complicating historical or philosophical dimensions, and most teachers find it much easier to teach from that perspective. That convenience, he concluded, will not be readily surrendered.

Patterson and Kuhn also noted that science majors may strongly resist the addition of the history of science to their training unless it will produce better credentials for entry into graduate school or professional employment. Gross (1980) also raised possible difficulties regarding teacher qualifications. Many physics teachers and professors, for instance, may require

extensive additional training in the history of science, the biological sciences, and possibly sociology and/or political science before they feel comfortable teaching science from the historical perspective.

Perhaps the most immediately important implication for science education, however, is inherent in Structure's emphasis on the theoretical incommensurability between competing paradigms and related problems of language across research traditions. From this perspective it is critically important that, when making reference to "what Aristotle, or Leonardo da Vinci, or Galileo said" for example, teachers make sure students understand how the terms were conceived within the different paradigms in which each perceived nature. Leonardo, for instance, is often cited for his contributions to modern science and technology, but seldom with any reference to the distinctly different world-view from which he perceived nature. For those not familiar with the distinction between the meaning of some key scientific terms in current physical theory and the now defunct impetus theory through which Leonardo conceived the relation of force and motion, this sort of reference may contribute to a perception of cumulative scientific development that, Kuhn insists, never existed. Given the fact that research by McCloskey (1983) and other cognitive psychologists indicated that the majority of subjects studied, even those well trained in the principles on modern science, perceived many common natural phenomena in terms more akin to medieval impetus theory than modern physics, research into this

dimension of Kuhn's thesis clearly should be a priority for a civilization that is clamoring for scientific literacy.

Conclusion

Structure has focused attention on several important questions regarding current science education practice. Chief among these is the issue of the history of science in science education, which has been the subject of increasing interest as a viable educational option over the past two decades. Given the apparent incompatibility of many facets of the current textbook tradition with a historical approach to science teaching, however, this constitutes a major challenge. Given the possible advantages to be gained from incorporating the history of science into the science curriculum, however, it is clear that this option deserves careful examination as the science education profession deliberates its direction in the coming decades.

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